Planned Experimentation & Quality Design

Version 1.0

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1. Introduction

Many researches have been conducted to define concepts and methodologies to ensure product quality. Almost all of them have one thing in common: statistical analysis on manufactured products called Quality Gates. While effective, they tend to be costly and reactive, and erode product gross margin. Other studies have been attempted to make product quality a proactive effort, among which are the Taguchi methods for experimental design. The Taguchi techniques form a structure for designed experimentation and have been found useful to push product/process quality considerations and targets farther upstream in the development process. Specifically, it is widely recognized that it is through engineering design the opportunity lies where the ultimate product delivery to customer needs can be greatly influenced, and, consequently, successful product launch and commercialization are highly probable in a well-crafted product introduction scheme.

The Taguchi approach to quality design has a number of significant strengths which ought to be exploited. In particular, Taguchi has placed a great emphasis on the importance of minimizing variation as a means to improve quality and of bringing the mean of the process to the design target. The idea of designing products whose performance is not sensitive to environmental conditions, and making this happen at the design stage through the use of design of experiments, have been cornerstones of the Taguchi methodology for quality engineering. However, understanding the Taguchi methodology requires fundamental understanding of some tools in statistics and the engineering experimental environment.

The purpose of this paper is not a lesson in statistics. Rather, it is an introduction to experimental design using statistical tools. The science underlying the tools and concepts is outside the scope of this paper. The objective is to promote experimental design and incorporate the concepts in the engineering process. Ultimately, the goal is to improve the quality of our products and reduce the cost of quality. This paper alone will not achieve this goal. However, it is a continuous effort on the part of product engineering, engineering management, and quality management to lay out a strategy for product development and technology transfer to High Volume Manufacturing (HVM). In addition, other papers on the subject matter should help introduce or advance the needed knowledge.

2. Experimental Design Considerations

2.1. Randomization & Blocking

System variations reduce the reliability of measurements. Internal or external variations constitute noise which may be transmitted to the output of the process. Known sources of variation should be included in the designed experiment to understand their influence in order to provide a more meaningful and significant assessment of the variables under study. Blocking is a technique for isolating system-level noise such as room temperature, certain machine setup, shift, operator, etc. An example of experiment blocking would be conducting a number of test runs under constant system conditions. Repeating the test runs under different system conditions is another block. This technique balances out environment variation.

Still, some other sources of variation may be present and inherent within and between test runs which may bias your results and which are of a random nature. Experiment test runs differ in their test conditions. Randomization refers to pairing and randomizing test runs for a given block of tests. In sum, a block is a set of a randomly run tests (see Figure 1).

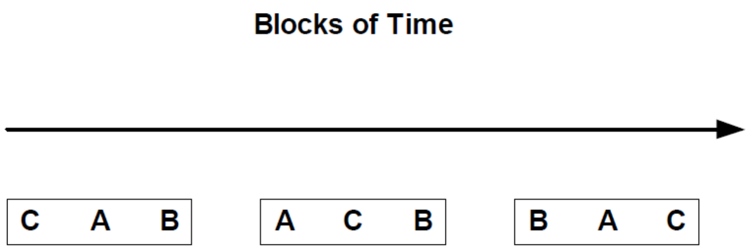

FIGURE 1. RANDOMIZING & BLOCKING TEST RUNS

2.2. Standard Error Estimate

In product/process parameter design, experimental testing is conducted in samples, of size n, of test runs in order to establish an average value for the parameter. When comparing the average values for each sample, one can be sure to recognize their variation. This variation in samples' averages follows a normal distribution if the parameter under study is normally distributed. To give an estimate of the parameter's "true" value, an average value for all samples' averages is used. This average value is an estimate (point estimate) of the true mean value of the parameter in a population, very large *n* (~ ∞).

When reporting a point estimate, it is usually desirable to give some idea of the accuracy of the estimate. Averages are inherently less variable than their corresponding data. Nevertheless, they do incorporate variation due to error in individual data points. The measure of accuracy employed is the standard error of the estimator.

The Estimated Standard Deviation or Error of $\hat{y}$ is:

$$SE(\hat{y}) = \frac{s}{\sqrt{n}} \qquad (1)$$

where s is the population's estimated standard deviation derived from a sample size n. To illustrate, consider the following example.

Example 1

A technical journal article describes a new method of measuring Overall Timing Accuracy of a signal-generating system. Using specific system setup and environmental conditions, the following 10 measurements of waveform edge placement accuracy (in ps) were obtained:

41.60, 41.48, 42.34, 41.95, 41.86,

42.18, 41.72, 42.26, 41.81, 42.04

A point of estimate of the mean edge placement accuracy is the sample mean:

$\hat{y}$ = 41.924 ps

Since the population standard deviation σ is not known, we may replace it by the sample standard deviation s = 0.284 to obtain the estimated standard error using eq(1):

$$SE(\hat{y}) = \frac{0.284}{\sqrt{10}} = 0.0898$$

Notice that the standard error is 0.2% of the sample mean, implying that we have obtained a relatively accurate point estimate. If we can assume that edge placement is normally distributed, then two times the standard error (analogous to 2σ) is 0.1796. Therefore, we would be highly confident that the true mean edge placement accuracy is within the interval

41.924 ± 0.1756 or between 41.744 and 42.104

This notion of parameter estimation accuracy is similar to point estimate with a 95% confidence interval. This is done using a multiplier, t, to estimate the parameter within 95% confidence level assuming that the population is normally distributed. The t value is obtained from the t distribution table (appendix A) using 2 parameters: α (α = 0.025 for 95%) and υ (degrees of freedom = n-1).

To apply the t distribution to example 1 in order to obtain the confidence limits for the "true" mean η we need to consult the t table for α = 0.025 and υ = 9. It is found that t = 2.262 and the 95% confidence limits:

$$\hat{y} \pm t * SE(\hat{y}) \qquad (2)$$

41.924 ± 2.262 * 0.0898 or between 41.721 and 42.127

Notice the slight difference in range of the two intervals above. It should be obvious that in the first technique the accuracy interval was slightly tighter. That is because we assumed a multiplier of 2, rather than 2.262. In order to use the t test to achieve the 2σ accuracy calculated in the first technique, we would have to have 60 degrees of freedom, i.e. 61 observations. In general, t ~ 2 for υ > 20.

2.3. Significance Testing (Student's t Distribution)

While this technique is largely used for hypothesis testing to judge whether any calculated estimates of a parameter effect (on system performance) are important or as a tool for establishing a level of confidence with not enough sample size, it is referred to here for assessing differences. Common test objectives include comparison between designs, samples, or products along the dimensions of selected variables. When comparing 2 sets of data, it is important to understand how significant the difference of their mean performance is. This is important for distinguishing the effect of key parameters between 2 designs, for example, by answering the question: Are they different? A condition for the applicability of the t-test is the normality of distribution of the phenomena under evaluation. Testing for normality of distributions can be done by conducting tests for normality. But the most effective is visual observation of a histogram. The following techniques provide two alternatives for performing significance testing.

2.3.1. Independent t-Test

The *t*-test is the most commonly used method to evaluate the differences in means between two groups. This technique pools the standard deviation for 2 samples to establish a reference standard deviation and characterizes the difference between the 2 samples using the difference between their means.

Example 2

Two designs (R and B) have been proposed as a solution to a product design problem. Test results have provided the following information:

$$n = 4$$

$$\hat{y}_R = 2.70,\ \hat{y}_B = 3.30$$

$$s_R = 0.27,\ s_B = 0.18$$

Of course, we can see there is a difference. But, is the difference important? To get a reference distribution, pool the variances:

$$s^2_p = (s^2_R + s^2_B)/2$$

$$s^2_p = 0.053 \rightarrow s_p = 0.23$$

where s_p is the pooled standard variation.

The Standard Error of a difference of 2 sample means, $\hat{y}_B - \hat{y}_R$, with no replication both with the same number, n, of observations is:

$$SE(\hat{y}_B - \hat{y}_R) = s_p * \sqrt{(2/n)} \qquad (3)$$

$$SE(\hat{y}_B - \hat{y}_R) = 0.23 * \sqrt{(2/4)} = 0.16$$

Therefore, the standard error for the difference is 0.16. What remains is to calculate the t ratio then make an assessment of the difference.

$$t = \frac{\hat{y}_B - \hat{y}_R}{SE(\hat{y}_B - \hat{y}_R)} \qquad (4)$$

Using eq(4), the t ratio is calculated to be 3.8. To make an assessment of the difference, this value is compared to tabulated t value for $\alpha = 0.025$. But we also need a value for the degrees of freedom, υ. Here, we have a special consideration for figuring out this value. Since the analysis involved 2 (m) separate sets of data of equal sample size, n, then

$$\upsilon = m\ (n-1) \qquad (5)$$

$$\upsilon = 2\ (4-1) = 6$$

Based on α and υ, the corresponding t value from the t distribution table is 2.447. To, finally assess the difference between the designs, the calculated t ratio is compared to the tabulated t value. If the calculated t ratio is *greater* than the tabulated one, then there is a significant difference between the 2 samples. In our example, that is shown to be true. Therefore, we have a reason to believe that the 2 designs are statistically different.

Alternatively, an assessment of the difference can be made by using eq(2), which would result in a calculated 95% interval of 0.6 ± 0.39. If zero is *in* the interval, then there is *no* difference. In our example, that is not true. Zero is not in the interval. Therefore, the designs are different.

2.3.2. Paired (Pairing and Diffing Runs) t-Test

To use this method of analysis, test runs must be paired because only one data set will be used for the analysis, unlike the independent t-test. The reason for this is the objective of testing for variation differences within groups instead of between groups. An example of this would be testing the difference in effect of manufacturing process variations on the performance of 2 designs, or evaluating the variation in a robotic product performance between different SW algorithms due to a certain degree of embedded randomness. Then, it would be important to consider randomizing and blocking test runs. This data set represents the mathematical difference between the paired runs for each block. The t-test is then performed on this data set to assess the difference.

Example 3

Two designs (R and B) have been proposed as a solution to a product design problem. Tests have been conducted as illustrated in Figure 2 and the results are as follows:

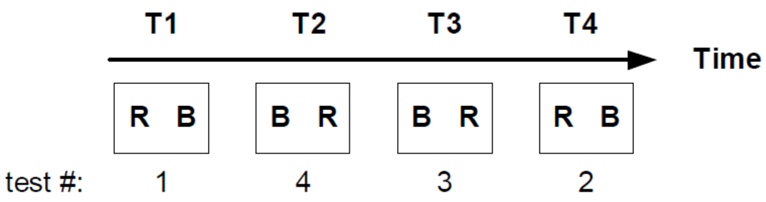

FIGURE 2. PAIRED TESTING

R: 2.7, 2.8, 2.7, 2.5
B: 3.3, 3.6, 3.2, 3.4

The resultant data set is,

y_{diff} = 0.6, 0.8, 0.5, 0.9 n = 4

From this, the following analysis parameters are calculated:

$\hat{y}_{diff} = 0.7$, $s_{diff} = 0.18$, $SE(\hat{y}_{diff}) = s/\sqrt{n} = 0.09$

Equation (2) will be used to assess the difference. Since the analysis involves one data set, then $\upsilon = 3$ and $\alpha = 0.025$ and the tabulated t value is 3.18.

The 95% interval is 0.7 ± 0.29. Zero is not in the interval; therefore, there is reason to believe that the 2 designs are different.

2.4. Factorial Experimental Design

Running experiments can be exhaustive, time-consuming, and expensive. The need for constructive experiments extends beyond the design of parameters (target values). The role of factorial design in achieving goals of "on target with smallest variation" has been clearly demonstrated by the Taguchi methods and is the essence of designed quality. However, these methods require fundamental knowledge and understanding of designed experiments which are the scope of this paper.

A multi-factor experimental design that deliberately varies the factors simultaneously is an art that has many advantages. In order to design quality into products and processes, the engineering process must incorporate engineering experimentation to understand which factors may affect performance and how these factors should be adjusted prior to final release of design. The need is for a systematic experimental strategy for testing out many factors efficiently, estimating their interaction effects, and making a technical assessment when choosing between alternatives. The 2-level factorial experiment is the simplest form of experimental design.

Perhaps the most important needs and uses for experimental design are choosing between alternatives, understanding the effect of key factors, reducing variations, hitting a target, maximizing/minimizing a response, or designing a robust process. This paper is mainly concerned with the choice of alternatives and reducing variations by making use of fundamental tools to build quality into non-complex product design.

Factorial experiments comprise some essential components. Constructing experimental environments require the appropriate selection of critical variables (factors), the design of experiment matrix, running the experiment and collecting data, data fidelity assessment, data analysis, and drawing conclusions.

Simple Example of Robust Design (On Target with Smallest Variation)

A certain injection molding machine setup is determined by 5 machine parameters which are likely to have some effect on part shrinkage. The control factors are

- x_1: nozzle temperature
- x_2: holding pressure
- x_3: holding time
- x_4: mold temperature
- x_5: screw speed

Also, 3 other non-machine-related factors are known to have an effect on part shrinkage:

- n_1: mold cooling water temperature
- n_2: percent regrind versus virgin raw material
- n_3: ambient temperature at the machine

Those 3 factors are somewhat difficult to control and so they are considered noise variables. To run the process on target with least variation, focus will be on the settings of the five control factors as one machine setup variable that minimizes part shrinkage and provide for the most consistently performing process in light of the presence of the three noise factors. Then, the objective of the experiment is to evaluate the effect of the 3 noise variables on process performance at 2 different (levels) machine setups. The goal is to identify a robust machine setup.

Process performance will be compared across only 2 levels of the machine setup variable, machine setup 1 versus machine setup 2. This is known as experiment blocking. It is *proposed* to run an experiment in which the conditions of the 3 noise variables are purposely varied at 2 levels (+ and -) for both of the machine setups. A two-level factorial design is proposed as a suitable matrix to define the variations of the noise variables.

Figure 3 shows a graphical representation of the design of the experiment where 16 trials are required for the proposed experiment. The cubes illustrate the operational region of the machine where the right angles are performance at corner conditions. The complete design matrix for this experiment is given in Table 1 where shrinkage values are given in 0.001 in.

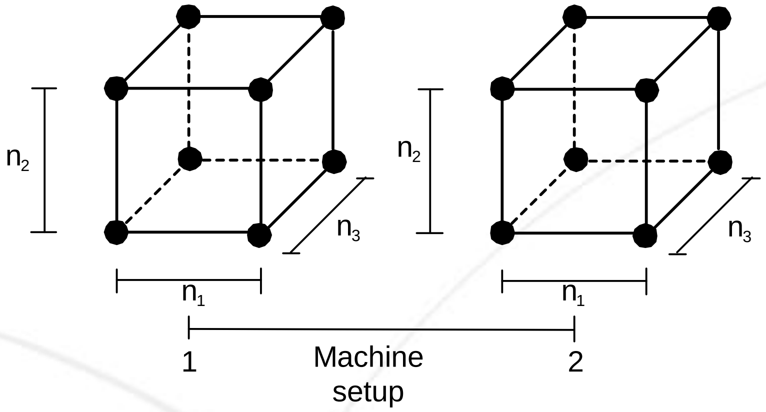

FIGURE 3. ROBUST DESIGN EXPERIMENT

Test	Machine Setup 1				Machine Setup 2			
	n_1	n_2	n_3	Shrkg. (y)	n_1	n_2	n_3	Shrnkg. (y)
1	-	-	-	10	-	-	-	7
2	+	-	-	10	+	-	-	10
3	-	+	-	12	-	+	-	15
4	+	+	-	11	+	+	-	11
5	-	-	+	10	-	-	+	11
6	+	-	+	12	+	-	+	8
7	-	+	+	11	-	+	+	12
8	+	+	+	12	+	+	+	14

TABLE 1. DESIGN MATRIX FOR ROBUST DESIGN

At the onset, to analyze the data from a robust design point of view, popular statistical metrics such as mean and standard deviation may be used to compare the two machine setups. For further characterization, analysis would extend to identifying the effect of each variable and the interaction effects between variables on the process performance, which is a full-fledged Two-Level Factorial Experiment. In both cases, analysis tools and methodologies described in this paper should provide for effective characterization.

3. Two-Level Factorial Experiments

A common experimental design is one with all input factors set at two levels each. These levels are called 'high' and 'low', or '+1' and '-1", respectively. A design with all possible high/low combinations of all the input factors is called a full factorial design in two levels. In general, if there are k factors, each at 2 levels, a full factorial design has 2^k runs.

Such experiments are designed based on special considerations that collectively constitute good practice in the design of experiments. No matter what the individual's experience is, experiments are not designed on-the-fly or drawn in the air. This is called shooting from the hip, or following the pattern: get set, fire, aim.

Because designed experiments involve high brain power at the planning stage, it usually discourages technical professionals due to the fact that much time is spent on non-value-adding activities such that efforts must continuously generate results, in addition to the type of data analysis involved which require special training and skill, and the possible cost associated. The result is that individuals rely heavily on their technical experience and start the action not well thought of. There are severe consequences to such strategies.

Experiments should be treated as projects because they consume resources, they run under certain requirements, and they have deliverables. Therefore, such experiments must be well defined and managed as well as executed. Experimental design should be easy to setup and carry out, simple to analyze and interpret, and simple to communicate or explain to others.

The objective of this section is to describe a certain experimental design technique and analysis using the previously described statistical analysis tools in designing products or processes. The goal is to help you understand designing for on-target performance prior to releasing the final version of the design to High Volume Manufacturing (HVM) and how to estimate the main effect of individual variables and interaction effects between the variables on overall system performance.

Nevertheless, while engineers structure their design and experimental practices, we must not ignore creativity. Not all experiments and testing need to be designed. Depending on the complexity and need, some trivial, random testing can produce valuable information not accounted for in the more designed experimental approach, depending on the complexity of the system.

The following example is a project I conducted in the fall of 2003 with a group of technical professionals. The data presented in the example are real-life data based on actual execution of the experiments.

Helicopter Robust Design: A Detailed Example

A toy manufacturer requested a recommendation by engineering for robust design parameters of a toy helicopter blades which will optimize flight time. The objective was to design and conduct an experimental program for the design of high performance helicopter propeller blades. As a result of project scope and requirements definition, engineering decided that by implementing a 2-level factorial experimental design the main and interaction effects of three variables deemed important in affecting helicopter flight time will be determined. The following is a description of the experiment process.

Experiment Design

The experiment was designed to evaluate the effect of three critical variables on helicopter flight time. First, a list of factors generally thought to affect a helicopter's flight time was generated. Then, relying on engineering common sense and general physics, a process of elimination was used to arrive at the top three critical factors listed in Table 2.

Parameter	Low	High
Rotor : Body Length Ratio	1:1	2:1
Ballast (added paper clip weight)	454mg	1361mg
Material Thickness	91µm	267µm

TABLE 2. PARAMETER LIST

In order to understand the influence of each factor as well as their interaction, a 2-level factorial experiment was constructed and executed such that a unique combination was identified for optimum performance. Two types of paper material were used representing thick and thin helicopter constructs. Half of each of those 2 types of paper material was built with a low body/rotor ratio and the other half high. Similarly, one paper clip, as a low ballast, for added weight was used at the bottom of the helicopter to provide center of rotation anchorage for increased stability versus three paper clips, as the high ballast level.

The experiment trials would be run in randomized standard order. The total number of runs would be $2^3 = 8$ runs using Fisher's idea of simultaneously varying all factors in a controlled manner, as shown in Table 3. The flight time for each run would be recorded accordingly and a 3-D cube plot generated, as shown in Figure 4. Based on the data and graph, assessments of variable main and interaction effects would be performed.

Standard Order	Rotor : Body Ratio	Ballast	Thickness
1	1:1	454mg	91µm
2	2:1	454mg	91µm
3	1:1	1361mg	91µm
4	2:1	1361mg	91µm
5	1:1	454mg	267µm
6	2:1	454mg	267µm
7	1:1	1361mg	267µm
8	2:1	1361mg	267µm

TABLE 3. TEST CONDITIONS

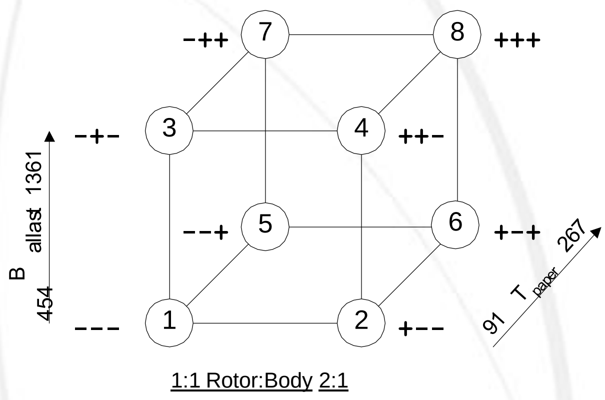

FIGURE 4. GEOMETRICAL REPRESENTATION OF THE TEST CONDITIONS

Setup & Procedure

After determining the three factors to be varied during the experiment, two patterns were created using an 8.5" x 11" sheet format, one pattern each representing the two rotor/body ratios. The low ratio consisted of both the body and the rotor length being 5" long (1:1) with 1" at the midsection of the copter, making up the total paper long dimension of 11". The high ratio consisted of the rotor length being twice that of the body length, or 6.66" to 3.33", while maintaining a 1" mid-section. Eight helicopters were then fabricated, four from each paper thickness defined above. Half of the fabricated copters of each thickness was of the low rotor/body variety, and the other half high rotor/body ratio. Next, either one paper clip (454mg) or three paper clips (1361mg) were attached at the bottom center section of each helicopter such that all permutations of the three factors were represented. Finally, each copter was labeled 1-8 corresponding to the standard order listed above for ease of recognition.

Using an Excel spreadsheet template that was developed in advance, the order of flight was randomized using a random number generator, and the design and calculation matrix was setup for data entry. One member of the group was designated to drop each helicopter, one to time each flight (in seconds), and one to enter the flight data into the spreadsheet template. The experiment was replicated twice for a total of 24 flights and data collected as shown in Tables 4 and 5, while results are illustrated in a behavioral model in Figure 5.

Variable	Characteristic	Low	High	Units
A	Rotor/Body Ratio	1:1	2:1	(none)
B	Ballast	454	1361	mg
C	Paper Thickness	91	267	um

TABLE 4. EXPERIMENT VARIABLES

Test	A	B	C	Y_1	Order	Y_2	Order	Y_3	Order	Y_{avg}
1	-1	-1	-1	6.00	8	6.70	3	5.60	7	6.10
2	1	-1	-1	5.60	3	6.00	6	5.20	3	5.60
3	-1	1	-1	6.10	7	6.30	1	6.20	8	6.20
4	1	1	-1	6.20	1	5.30	4	5.60	1	5.70
5	-1	-1	1	3.80	5	3.80	7	4.00	5	3.87
6	1	-1	1	3.40	2	3.50	5	3.80	6	3.57
7	-1	1	1	4.00	6	4.00	2	4.00	2	4.00
8	1	1	1	3.40	4	3.10	8	3.40	4	3.30

TABLE 5. EXPERIMENT DESIGN MATRIX

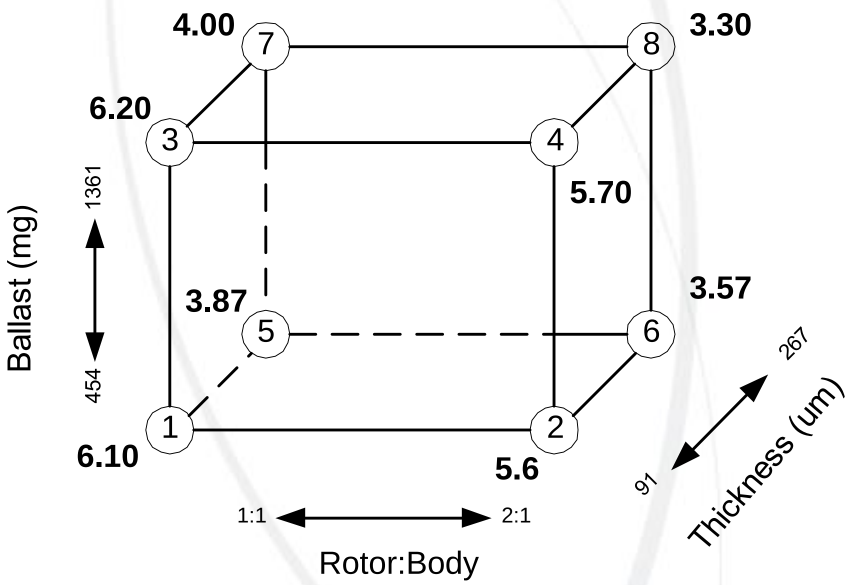

FIGURE 5. GEOMETRICAL REPRESENTATION OF THE TEST RESULTS

This behavioral model can be used to visually observe the effect of each variable. Figures 6 to 8 illustrate the result, where variables A and C can be shown to have an effect.

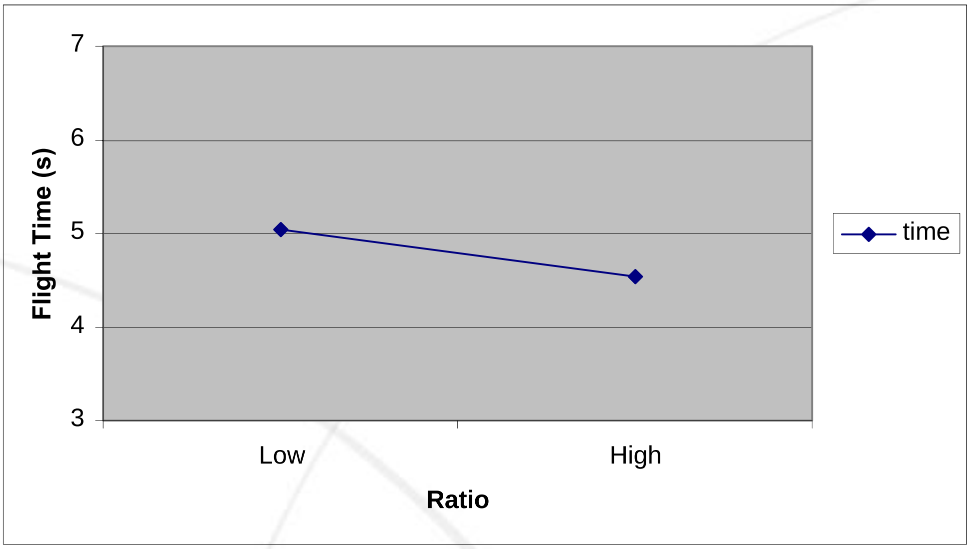

FIGURE 6. RATIO (A) MAIN EFFECT

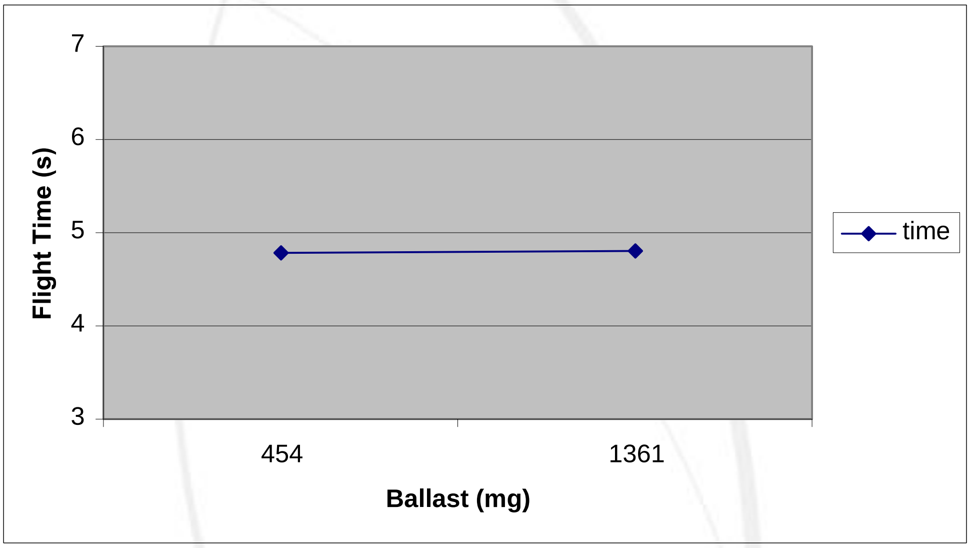

FIGURE 7. BALLAST (B) MAIN EFFECT

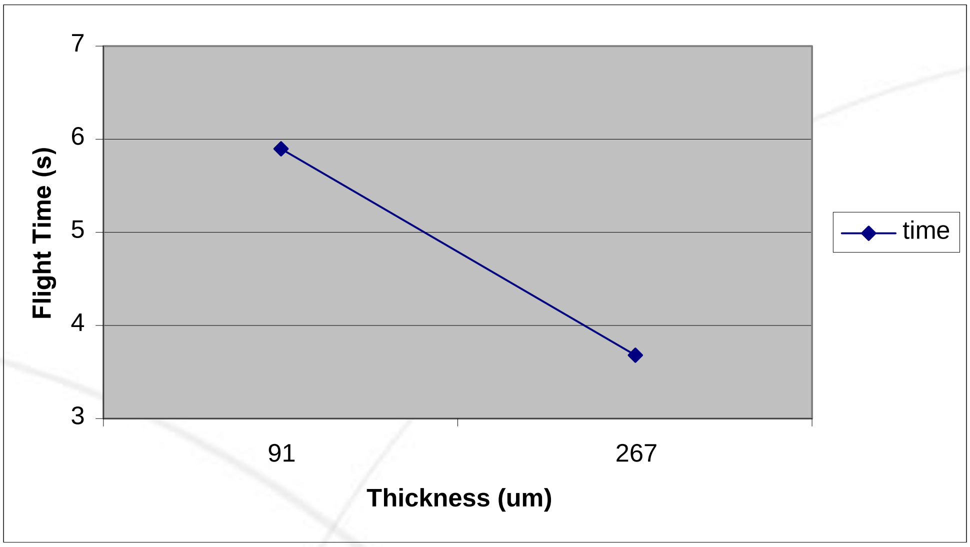

FIGURE 8. MATERIAL THICKNESS (C) MAIN EFFECT

Main (E_i) and Interaction Effects

The calculation matrix represented in Table 6 shows the individual and combinational variable levels for each test. The column on the far right is the average response.

A	B	C	AB	AC	BC	ABC	Y_{avg}
-1	-1	-1	1	1	1	-1	6.10
1	-1	-1	-1	-1	1	1	5.60
-1	1	-1	-1	1	-1	1	6.20
1	1	-1	1	-1	-1	-1	5.70
-1	-1	1	1	-1	-1	1	3.87
1	-1	1	-1	1	-1	-1	3.57
-1	1	1	-1	-1	1	-1	4.00
1	1	1	1	1	1	1	3.30

TABLE 6. CALCULATION MATRIX

The interaction effects levels are the mathematical product of the main effects. This is a useful technique that is adequate for estimating interactions rather than having to build prototypes at the required level of each variable, which would have required 8 additional prototypes. The scientific proof of this technique is outside the scope of this paper. The next step is to calculate the numerical value of these effects.

Test	A	B	C	AB	AC	BC	ABC
1	-6.10	-6.10	-6.10	6.10	6.10	6.10	-6.10
2	5.60	-5.60	-5.60	-5.60	-5.60	5.60	5.60
3	-6.20	6.20	-6.20	-6.20	6.20	-6.20	6.20
4	5.70	5.70	-5.70	5.70	-5.70	-5.70	-5.70
5	-3.87	-3.87	3.87	3.87	-3.87	-3.87	3.87
6	3.57	-3.57	3.57	-3.57	3.57	-3.57	-3.57
7	-4.00	4.00	4.00	-4.00	-4.00	4.00	-4.00
8	3.30	3.30	3.30	3.30	3.30	3.30	3.30
Effect	**-0.50**	**0.02**	**-2.22**	**-0.10**	**0.00**	**-0.08**	**-0.10**

TABLE 7. MAIN AND INTERACTION EFFECTS ESTIMATES

Table 7 shows the estimates for each effect type. The value in each cell of the matrix is the product of the variable's level (+1 or –1) and the corresponding average response for the particular test, Y_{avg}. To calculate the numerical value of an effect, sum its values and divide by half the number of tests, i.e. 4. That is because each level was exercised 4 times throughout the tests.

Independent t-Test

In order to determine which effects were significant, a calculation of variance of individual observations was performed and an independent t test conducted as follows:

Ave	S2i
6.10	0.31
5.60	0.16
6.20	0.01
5.70	0.21
3.87	0.01
3.57	0.04
4.00	0.00
3.30	0.03

$s^2_p = \Sigma s^2_i/m$ (m=8) $s^2_p = 0.10$
$s^2_{effect} = 4s^2_p/N$ (N=24) $s^2_{effect} = 0.02$
$s_{effect} = 0.13$

Recall from section 2.3.1 that the independent t test requires pooling the variances and a reference, pooled variance calculated. This experiment represents a unique condition for calculating the Standard Error (SE) of an effect, s_{effect}. Equation (3) represented the standard error calculation for a non-replicated experiment or a mean value of the data set. The Standard Error of an effect in a replicated 2^k factorial experiment is,

$$SE(E_i) = \sqrt{(4\, s^2_p / N)} \qquad (6)$$

where N is the total number of experiments, including replicates.

Effect (E_i)		t_e (= E_i/s_{effect})	Significant Effect ($\lvert t_e \rvert > t$)
A	-0.5	-3.93	Yes
B	0.02	0.13	No
C	-2.22	-17.43	Yes
AB	-0.1	-0.79	No
AC	0	0	No
BC	-0.08	-0.66	No
ABC	-0.1	-0.79	No
Average	4.79	t (v=16, 95%)	**2.12**

To calculate the degrees of freedom (DOF) eq(5) was used. Here, each test is a unique set of data of sample size n = 3, and there are 8 sets. So, the experimental degrees of freedom is 16.

As can be seen from the table above, main effects A and C have been shown as significant. They demonstrated higher t values than the *t distribution* with 16 degrees of freedom. Calculating the 95% confidence interval for each effect estimate would show that A and C demonstrate significant difference than the rest since their interval would not include zero.

Therefore, the engineering recommendation would be to focus on the rotor/body ratio and material thickness for a robust design and that there would be no significant interaction effects between those design parameters. Not only that, but the ballast would be irrelevant in design robustness.

The 2-level factorial experiment helped to identify design attributes in performance assessment efficiently. From the preceding analysis it can be concluded that material thickness considerably affected performance followed by rotor-to-body ratio. The focus was narrowed down on those two factors due to their apparent significance. The analysis would have to be more exhaustive to evaluate each effect. Nevertheless, the results have established that interaction effects were minor compared to those more significant main effects. The cube diagram would be the most appropriate tool for the evaluation.

In a more realistic, complex development environment such conclusions would have a significant economic and technical impact on the development project and the product with a desirable impact on the marketing community. It would be the impact of facilitating the decision making process with much confidence that would lead to successful product development and commercialization, all other factors being constant. With a limited number of prototypes of different, controlled design parameter values, much information can be obtained at the early stages of prototype iterations that indicate where the target value lies with high degree of confidence and relate that to product requirements and project objectives. The ultimate result is a shortened, highly confident product development process and a high quality product.

Appendix A

PERCENTAGE POINTS OF THE T DISTRIBUTION

Tail Probabilities

	One Tail	0.10	0.05	0.025	0.01	0.005	0.001	0.0005	
	Two Tails	0.20	0.10	0.05	0.02	0.01	0.002	0.001	
D	1	3.078	6.314	12.71	31.82	63.66	318.3	637	1
E	2	1.886	2.920	4.303	6.965	9.925	22.330	31.6	2
G	3	1.638	2.353	3.182	4.541	5.841	10.210	12.92	3
R	4	1.533	2.132	2.776	3.747	4.604	7.173	8.610	4
E	5	1.476	2.015	2.571	3.365	4.032	5.893	6.869	5
E	6	1.440	1.943	2.447	3.143	3.707	5.208	5.959	6
S	7	1.415	1.895	2.365	2.998	3.499	4.785	5.408	7
	8	1.397	1.860	2.306	2.896	3.355	4.501	5.041	8
O	9	1.383	1.833	2.262	2.821	3.250	4.297	4.781	9
F	10	1.372	1.812	2.228	2.764	3.169	4.144	4.587	10
	11	1.363	1.796	2.201	2.718	3.106	4.025	4.437	11
F	12	1.356	1.782	2.179	2.681	3.055	3.930	4.318	12
R	13	1.350	1.771	2.160	2.650	3.012	3.852	4.221	13
E	14	1.345	1.761	2.145	2.624	2.977	3.787	4.140	14
E	15	1.341	1.753	2.131	2.602	2.947	3.733	4.073	15
D	16	1.337	1.746	2.120	2.583	2.921	3.686	4.015	16
O	17	1.333	1.740	2.110	2.567	2.898	3.646	3.965	17
M	18	1.330	1.734	2.101	2.552	2.878	3.610	3.922	18
	19	1.328	1.729	2.093	2.539	2.861	3.579	3.883	19
	20	1.325	1.725	2.086	2.528	2.845	3.552	3.850	20
	21	1.323	1.721	2.080	2.518	2.831	3.527	3.819	21
	22	1.321	1.717	2.074	2.508	2.819	3.505	3.792	22
	23	1.319	1.714	2.069	2.500	2.807	3.485	3.768	23
	24	1.318	1.711	2.064	2.492	2.797	3.467	3.745	24
	25	1.316	1.708	2.060	2.485	2.787	3.450	3.725	25
	26	1.315	1.706	2.056	2.479	2.779	3.435	3.707	26
	27	1.314	1.703	2.052	2.473	2.771	3.421	3.690	27
	28	1.313	1.701	2.048	2.467	2.763	3.408	3.674	28
	29	1.311	1.699	2.045	2.462	2.756	3.396	3.659	29
	30	1.310	1.697	2.042	2.457	2.750	3.385	3.646	30
	32	1.309	1.694	2.037	2.449	2.738	3.365	3.622	32
	34	1.307	1.691	2.032	2.441	2.728	3.348	3.601	34
	36	1.306	1.688	2.028	2.434	2.719	3.333	3.582	36
	38	1.304	1.686	2.024	2.429	2.712	3.319	3.566	38
	40	1.303	1.684	2.021	2.423	2.704	3.307	3.551	40
	42	1.302	1.682	2.018	2.418	2.698	3.296	3.538	42
	44	1.301	1.680	2.015	2.414	2.692	3.286	3.526	44
	46	1.300	1.679	2.013	2.410	2.687	3.277	3.515	46
	48	1.299	1.677	2.011	2.407	2.682	3.269	3.505	48
	50	1.299	1.676	2.009	2.403	2.678	3.261	3.496	50
	55	1.297	1.673	2.004	2.396	2.668	3.245	3.476	55
	60	1.296	1.671	2.000	2.390	2.660	3.232	3.460	60
	65	1.295	1.669	1.997	2.385	2.654	3.220	3.447	65
	70	1.294	1.667	1.994	2.381	2.648	3.211	3.435	70
	80	1.292	1.664	1.990	2.374	2.639	3.195	3.416	80
	100	1.290	1.660	1.984	2.364	2.626	3.174	3.390	100
	150	1.287	1.655	1.976	2.351	2.609	3.145	3.357	150